

Interactive comment on “Exploring uncertainty in glacier mass balance modelling with Monte Carlo simulation” by H. Machguth et al.

H. Machguth et al.

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Response to the reviews and short comments on the paper "Exploring uncertainty in glacier mass balance modelling with Monte Carlo simulation" by H. Machguth, R. Purves, J. Oerlemans, M. Hoelzle and F. Paul

We would like to acknowledge both reviewers for their careful and constructive reviews of our paper and R. Dadic for her interesting short comment. Based upon the comments of the reviewers and of R. Dadic we have modified our paper to clarify a number of points, and carried out one further analysis. Where we feel that a point made should not be addressed in the manuscript, we have explained why that is the case here.

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"The only questions that could arise are - is there a possibility to derive temperature lapse rates from all the four stations in the vicinity of the glacier, e.g. by kind of a spatial interpolation considering topography?" Such an interpolation is unlikely to be successful, on the one hand because the number of available stations is rather small and on the other hand because assessing local effects at different stations and related representativeness of these stations for Morteratsch glacier would be a challenging problem that is beyond the scope of this paper..

"A short discussion of the effect of glacier dynamics could be included: how far have the AWS and the stakes moved downvalley during the 11 years of observation?" In particular at the point AWS downwasting dominates and horizontal flow is marginal. Thus, the latter can be disregarded.

"Pg.451, lines 15-17: Temperature and global radiation are mentioned as the two key inputs in the mass balance models,..." As the referee rightly points out, uncertainties in precipitation were treated in a simplified manner and they were not varied for the experiment shown in Table 2. We have added two comments in the paper to explain why we have decided to treat precipitation in a simplified manner. In our opinion, uncertainties in precipitation are more complex than those addressed in other parameters in the paper and should be addressed in detail in future studies.

"Pg. 455, section 3.2: It is not completely clear to me from the text if P_{corr} is applied uniformly at all the stations or is tuned for each station individually." The correction factor is adjusted at the point AWS and then applied uniformly to all points. We have clarified this in the text.

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"Pg. 457, lines 10-15: *It is a bit 'suspicious' that systematic error for temperature in only 0.3°C. One would expect...*" The uncertainty of 0.3°C refers only to the uncertainty of the measurement at the synoptic weather stations. The total uncertainty at the point AWS is further increased, as suggested by the referee, by the uncertainty in the lapse rate. We added a short explanation on how total uncertainty in air temperature at the AWS is calculated.

"Pg. 457, lines 22-25: *Since P is tuned by means of P_{corr} it would be good to include P_{corr} also in the sensitivity analysis. It is of interest...*" We agree that the uncertainty in precipitation is a major issue and should be addressed in future work. We added a comment to this effect to the conclusions of the paper. Furthermore, we explained why we have not focused on uncertainty in precipitation in detail (see first comment).

"Pg.461, section 4.2.8.: *How is the snow albedo treated in the model and why is there no sensitivity analysis for it?*" The uncertainty in the parameterization of the snow albedo is not considered. We have added a short explanation that the uncertainty analysis is not complete and have listed three further sources of uncertainties not considered in this paper.

"Pg. 465, line 10-15: *The model response is evaluated to different levels of uncertainty in $S_{in,meas}$ and T_a . Since the mass balance ...*" This comment seems closely related to the first comment. We have added an explanation that our uncertainty analysis is not complete, i.e. we do not consider all possible sources of uncertainty.

"Pg. 469, section 6.4: *I suggest shortening or even excluding the introduction sentences ...*". We agree that this part of the text can be seen as a repetition. However, we want to emphasise this point and consequently, we would like to leave these sentences as they are.

"Pg. 469, lines 20-22: *The sentence: 'In other words,...values are sensible.' does not read clear enough. Reformulation (just make it simpler) would be appreciated.*" The sentence mentioned by the referee is directly related to the following sentence. We

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have made the argument clearer by connecting these two sentences.

"Table 2: It would be more effective for the reader if these results are plotted (e.g. bar plot), or if the results are sorted starting with the parameter with the largest uncertainty." We fully agree and have replaced the table with a bar plot as suggested.

"Pg. 470, Conclusions: Considering the future work mentioned in the text I would suggest additional question to be explored: What is more ..." We agree on this point and have added this point to the conclusions.

Interactive Comment by R. Dadic:

"L10, P457: You introduce a measurement error of 0.3 °C for you air temperature, as well for the systematic error. Since your station ..." The same question was raised by Referee #2 and we would like to refer to the explanation given above.

"Section 4.2.3. It would be interesting to see what the errors would be in the model run without tuning the precipitation. If you have models ..." This is a very interesting point and a similar questions, concerning the tuning parameter P_{corr} has been raised by Referee #2. We have performed an additional model run to calculate mass balance at the AWS without tuning. The result is now also shown in the paper and indicates a change in mass balance of 0.6 m w.e.

"Section 4.2.6.: Why didn't you compare the temperature from the Meteoschweiz with the temperatures from the AWS on the glacier to estimate the lapse rate deviations when you actually use the glacier AWS?" The model is driven by data from measurements performed outside of the glacier boundary layer. From these measurements, energy fluxes are parameterized directly, i.e. air temperatures inside the glacier boundary layer are not calculated and thus, a direct comparison would not help to assess uncertainties. We have made this clearer in the revised manuscript.

"Is there a reason why you did not introduce an error for wind speed ..." Wind speed

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is not an input parameter to the model. We added an explanation as to how turbulent fluxes are parameterized in the model. Moreover, this comment also raises important questions as to the uncertainties inherent in these parameterizations. In the model applied, they have been considered in a simplified manner by assigning uncertainty to the related tuning parameter C_b .

Interactive comment on The Cryosphere Discuss., 2, 447, 2008.

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2, S331–S335, 2008

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